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Clayton J. Thomas Interview (MORS)

Thomas, Clayton J.

Military Operations Research Society (MORS) Oral History Project Interview of Clayton J. Thomas, FS

by Gene Visco, FS and Dr. Robert S. Sheldon, MORS President Elect

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MILITARY OPS RESEARCH HERITAGE ARTICLE

MR. VISCO: This is an interview with Mr. Clayton J. Thomas under the auspices of the Oral History Project of the Military Operations Research Society (MORS). The MORS Oral History Project is documenting the origins and early experiences of military operations research in the United States.

Today is the 29th of December 1998. The interview is being conducted in the office of Mr. Thomas, temporarily located at 1777 Kent Street, Rosslyn, Arlington, Virginia. The temporary location results from the renovation of the Pentagon building where Mr. Thomas is normally located.

My name is Gene Visco. Participating with me in the interview of Mr. Thomas is Dr. Robert S. Sheldon, MORS president-elect 1998–1999.

Clay, if you want to start off saying who are you and what is your job title, and that sort of background stuff.

MR. THOMAS: My name is Clay Thomas. I am the chief scientist, officially, I think it is senior scientist, but we call it chief scientist, of the Air Force Studies and Analyses Agency. I have a Pentagon address, Headquarters, USAF, 1570 Air Force Pentagon in Washington, D.C. 20330. My phone number now is (703) 588-6944. My E-mail address is clayton.thomas@pentagon.af.mil, which indicates my Air Force affiliation.

You asked about my background. I was born, raised, and educated in St. Joseph, Missouri. I attended kindergarten through Grades 12, and then two years of junior college, (Grades 13 and 14). Then I won a scholarship to the University of Chicago in 1940, attended Chicago for a couple of years, majored in math, minored in physics, got a Bachelor's in '42.

In the spring of '42, I enlisted in the Army Air Force, (not the Air Corps, but by that time the *Army Air Force*), became a cadet for nine months, and upon graduation was commissioned as a second lieutenant in the Army Air Force. I was in the Army Air Force as a weather officer from '42 through the fall of '45.

MR. VISCO: How did you happen to take mathematics and physics? Was there something that led you to the sciences: family, or your predilections?

MR. THOMAS: Yes. My parents had considerable influence on me although they had very little formal education. My father had about six years of schooling in a German-speaking community in Kansas. He knew no English until he was about six and went to school.

My mother also grew up in Kansas. She graduated from high school, and she taught school in a nearby community in Nebraska for a year or so, and she went about one year, I think it was, to a business college.

They were both quite intelligent. My father was very sharp, very careful, was a stickler for accuracy, and had keen appreciation for cause and effect relationships. My mother learned to read when she was about three, read voluminously, had a great deal of general knowledge. She had been very good in math and in that area I inherited great genes, and I found mathematics to be a fascinating subject, but I liked most subjects.

DR. SHELDON: That scholarship to Chicago, how did you happen to win that?

MR. THOMAS: We had to take a written test, and I was pretty good at taking tests, leveraging a little bit of knowledge to make better-than-average guesses on multiple choice questions.

After the war, I went back to the University of Chicago in January of '46. I got my Master's in math in '47, and continued taking graduate math courses, I had all my courses required for a Ph.D. plus language exams.

Then, I started working part time on a project at the University of Chicago. There was some talk about having classified dissertations, but that never materialized, so I never got a Ph.D. (though the older I became more and more called me "doctor").

MR. VISCO: You are one of those ABD's.

MR. THOMAS: Right.

Some of my experiences while growing up led me into OR type activities. I had always been fascinated by measuring devices, e.g. carpenters rulers. I asked for and received a stopwatch as a present.

I felt that sports, for example track and field, had many aspects where scientific approach could be of value. I measured off the alley behind our house, 201 yards long, used to organize races there, and speculated with some of my friends how one could apply science to various sports including baseball. Also, when we took family trips, I recorded our progress, so I could try to relate our average speed to several variables.

MR. VISCO: I oftentimes time aircraft takeoffs from the time the engines rev up until the time the wheels lift off. The average is 20 seconds.

MR. THOMAS: I felt that my work in studying meteorology and trying to forecast weather gave me a good example of an

area which has all kinds of mathematical or scientific trappings, but where there is a great deal of art involved in actual forecasting, which was a very imperfect profession in those days (and still far from perfect). In the same vein I found useful examples in two fascinating but non-rigorous books that I read once I was out of the 8th grade and had a card to the upstairs part of the St. Joseph library—*Calculus for the Practical Man* and *Statistics for Students of Psychology and Education.*

My first formal involvement in military operations research came when I started working part time in a project at the University of Chicago in 1947. It was called Project CHORE. The meaning of that, at the time, was classified Secret. Now that it is completely unclassified, I can tell you that "CHORE" meant Chicago Ordnance Research. It was sponsored jointly by the Army, Navy, and Air Force. Later the Air Force became the sole sponsor and renamed CHORE as the Institute for Air Weapons Research, IAWR.

Our task was, as the names implied, to study ordnance and its impact on one-on-one air combat. Later, we got into a little bit of a campaign analysis, but not very sophisticated, where we used Lanchester equations, zero-sum two-person game theory, etc.

DR. SHELDON: Were there members of the faculty involved or was that a post-doc?

MR. THOMAS: Yes. The dean of the Division of Physical Sciences, Dr. Walter Bartky, was the director. For a while we had an assistant director, Dr. Ed Hewett, who was a mathematics professor specializing in topology. He had been an operations researcher in World War II, had flown on half a dozen bomber missions, and his expertise was aerial gunnery. He taught courses to aerial gunners in rules for aiming in combat, often non-intuitive to those with hunting experience.

We had an associate director, who was a retired Army lieutenant colonel who lost a leg in a C-54 accident in Alaska. He thought the sun rose and set at Wright Patterson Air Force Base.

DR. SHELDON: Do you remember that Army colonel's name?

MR. THOMAS: Yes, Paul Shanahan. He was quite knowledgeable about operations, and that was useful.

However, the first time I was ever involved in an Air Force study where the client indicated what he thought the answer should be, came back when I was base weather officer at Laughlin Field (now AFB) in Del Rio, Texas. One afternoon, Major Avery came striding into the weather station. He said, Captain Thomas, "I need your help, I want to get out of this doggone PT (physical training) in 100-plus-degree temperatures." He said he had heard that the same isotherm goes through both Del Rio and Death Valley, and he wanted me to do a study that would get us out of PT.

Well, I told him we would find out what the facts were. He was right about the isotherm. I wrote that up, but we didn't get out of PT. As a matter of fact, I was coming out of PT one afternoon when we heard about the death of Franklin Roosevelt.

Getting back to our project in Chicago, we were concerned about aircraft vulnerability and what the impact would be in interceptor versus bomber combat, etc. Since we were concerned with future aircraft, we didn't have any combat data, so we couldn't apply the same methods that had been used so effectively in World War II. We resorted to the theory of games to incorporate the effect of the times of fire; when one fired early he was more likely to be alive, but if firing later ranges would be less and if still alive his guns should have more effect. Using game theory we developed a lot of formulas for optimal firing times. That was exciting work.

DR. SHELDON: The aircraft vulnerability you were estimating, was it the vulnerability of the bombers or the interceptors or both?

MR. THOMAS: It was both. And the search for data and methodology led to several trips. We were going to evaluate the effectiveness of the B-52 versus Soviet interceptors. Of course, we were also interested in U.S. interceptors versus Soviet aircraft. We had a study involving that which came out in 2 May 1949.

I remember the first trip I was on. Several of us went to Wright Patterson Air Force Base, and we were briefed on the futuristic B-52, which had not yet come into service. We had briefings on the B-47, which had come in service, and a few other aircraft. That was in the fall of 1947.

In January of '48, two of us went to Aberdeen Proving Ground to find out about their program on vulnerability. They had a bunch of aircraft at which they fired different ordnance under a variety of conditions, and estimated what the effect of the damage would be on aircraft ability to fly. We tapped into the expertise of Herbert Weiss and Art Stein who

worked for Herb at Aberdeen. Both of them had considerable OR experience, which was very useful to us.

In December of 1948, 50 years ago, I made my first visit to RAND and talked to many of their experts on the theory of games, and got several documents. We were using the theory of games to find optimal firing times for estimating the probabilities of kill needed for the study due in 1949.

Later on, we also did studies on the tradeoff in designing a bomber between putting weights into turrets and guns as opposed to weights we devoted to bomber protection. We also did a short study on evaluating the wisdom of the Soviets in going to a more rapid firing gun that had been displayed in one of their May Day celebrations.

DR. SHELDON: I have a question on your study of turrets and guns, armament versus protection. Do you remember any insights that you provided to decision-makers from that study?

MR. THOMAS: Yes, we showed them tradeoff curves and some diagrams for allocating different amounts of weight.

As a different kind of effort, we participated in an air defense exercise in 1951, centered at Hamilton Field north of San Francisco. It was one of the key installations. Later we were involved in analyzing combat between our interceptors and Soviet interceptors used by the Chinese pilots. We had gun camera film and got some additional information from "supplemental combat information reports" or SCIR's. There was quite a bit of methodology development in those days, and that was one of the interesting motivations.

DR. SHELDON: What did you learn from looking at the combat films from Korea? Did you watch several of those?

MR. THOMAS: Yes. We got some insight into the ranges of firing and their varying combat effectiveness. There was also some air-to-ground film. The Germans had done some work using combat film in World War II and that was also useful.

I guess the first time I came across the term operations research was in reading Morse and Kimball, but the spirit of it was right down my alley.

DR. SHELDON: When did you first read Morse and Kimball?

MR. THOMAS: It was around 1950, I guess, thereabouts.

DR. SHELDON: Was that an unclassified or still classified version then?

MR. THOMAS: Still classified although there was a volume that Wald had written on the analysis that had been classified during the war, and one of our security advisers thought it still was, but I told him there was a declassified version in the Eckhart mathematics library on the campus at the University of Chicago.

DR. SHELDON: That was a text by Wald?

MR. VISCO: He had worked at one of the Army Air Force's Operations Analysis Sections, but as a younger man, wrote some textbooks afterwards.

DR. SHELDON: When you worked with Ed Hewett and others in Chicago, did they relate any of their World War II operations research experiences, bring it to the studies?

MR. THOMAS: To a certain limited extent. Walter Bartky was very expert in applied mathematics and statistics. As a matter of fact, he had done some early work on quality control, that he had not published, and didn't get much credit for it, but he was one of the very early pioneers. Later he became a vice president, University of Chicago. He was also a friend of the Northrups, which was useful.

We encountered a lot of the pioneers. I mentioned Herb Weiss and Art Stein at Aberdeen. At RAND, there were game theorists Lloyd Shapley (son of the astronomer), Ed Quade, Mel Dresher, Sam Karlin, John Williams, and a whole host of others.

At Project CHORE, there were a few division chiefs who had been involved in World War II analysis, Tom Caywood who was later an editor of the Operations Research Journal, Bob Porter, and Frank Bothwell. Jay (Jacinto) Steinhardt at OEG, which later became CNA, was an advisor of CHORE. There were many Army experts that had prior experience like Ellis Johnson, and some had been involved in World War II analysis for the Army Air Force.

At CHORE we were fortunate in having Dr. Bartky who had a lot of applied math experience, and had learned many tricks of the trade in mathematics. For example, one of his papers first introduced me to generating functions that have been very useful tools in statistics and probability.

MR. VISCO: Did Bartky have some World War II experience in OR that you know of?

MR. THOMAS: I am not sure how much experience he had. He may have been involved in some of the mathematical aspects.

I think one of the important things about our early work was that we wrestled with the problems of going to systems analysis as a generalization of operations research, because we were dealing with future combat systems where we had no actual data yet, so we had to use analogies, relevant data, relevant exercises, tests, extrapolations, but there was inherently less certainty about it. Then, there was not operations research work where you had some actual data for the real systems. That was probably particularly true of human performance.

I had not heard the term systems analysis when doing some of the early work at Chicago. I recall one time I was so concerned about the limitations that I spent most of one night writing a little credo of what I believed about what we were doing and how accurate or inaccurate it might turn out to be, and later I discovered that RAND had been bothered by the same thing that had troubled us, and had coined the term systems analysis.

DR. SHELDON: How did you convey uncertainty to the decision-makers, the people you were doing work for?

MR. THOMAS: We indicated what factors we had (and had not) considered, and we addressed the concerns that varied from individual to individual.

Some of the officers that we had briefed were aware of the many peculiar characteristics of World War II systems; e.g. there were some airplanes that turned left better than they turned right, and some officers asked if we had taken that kind of thing into account, and we indicated whether we had or had not, and whether we thought it was very relevant to results.

MR. VISCO: In some of those early studies, were you able to see the results, the implementation of recommendations or outputs of the study, were you ever aware of something actually being done as a result of one or the other of the studies?

MR. THOMAS: I think our work had some impact on allocation of effort. I don't know how well we got results that would serve as predictors. Some of the major benefits of the work was in formulas we had devised that became rather standard. Other people had either come across them in reports or derived them independently.

A lot of what we did was spreadsheet analysis, using classical spreadsheets. At one time I supervised about half a dozen girls who operated desktop calculators—Fridens, Marchands, Monroes. I don't think the game theory applications ever came into general usage but there has been a modest amount of use.

MR. VISCO: Some of the early ORO work, about the same time that you were doing some of this in Chicago, was attempting to relate game theory to operational gaming to see what kind of a link there was because the idea was that operational gaming was limited to one-time kinds of events. The idea was to see whether or not you could use game theory to sort of generalize from operational gaming, but that never got very far either.

MR. THOMAS: That came a little bit later. There was great enthusiasm for operational gaming in the late '50s and early '60s both at ORO and at RAND. Walt Deemer, who was the one who brought me to the Pentagon in Air Force Operations Analysis Office, was concerned about the validity of some of the conclusions people drew from gaming. He and I together wrote a paper which won the Lanchester prize on evaluating *The Role of Operational Gaming in Operations Research.* That was in the late fifties, around '58, I believe.

MR. VISCO: Zimmerman's paper on Monte Carlo, the use of Monte Carlo processes for combat simulation was the prizewinner in '56, and that was for the same general arena because everyone was suggesting how one would use the digital computer to supplement both closed form analysis and operational gaming.

MR. THOMAS: Our conclusion was that gaming was very useful in getting a feel for a subject, in developing some intuition, coming up with conjectures as to what might be true, but it would take something more like game theory to establish the optimality of strategies.

Also, among the pioneers were many active in professional societies. In the meetings of the first five years of ORSA (Operations Research Society of America) there was always a panel discussing what the definition of operations research (OR) should be, and there would be another panel asking if it was really possible to train people to do OR, and by training, they had in mind continuing the team approach which had been used in World War II.

We (the Air Force) had people of various disciplines on teams, even lawyers and people like Larry Starkey whose major was in English, but they all did good work, as well of course as

the physicists, chemists, mathematicians, and engineers of all persuasions.

Of course, the general definitions of operations research in those days usually emphasized its important basis in the scientific method like the established sciences, and there was quite a bit of influence of the scientific approach in the early days.

There was really a thrill, I think, and excitement in applying scientific method in areas where it had not yet been much used in getting results.

DR. SHELDON: Can you give me an example of where you applied the scientific method where it hadn't been applied before early on?

MR. THOMAS: Of course, World War II operations research and our extensions in doing systems analysis, involved areas that had had very little prior use of the scientific method. There had been some war gaming in World War I. Thomas Edison had done some, and there has been some gaming done by some of the people who later went to ORO, that anticipated some of the Pacific campaigns. It had been useful in developing intuition.

MR. VISCO: One anecdote about that. Ellis Johnson was at Pearl Harbor in the early part of December 1941. He was there to carry out some degaussing experiments for some of the Navy ships, so he was there when the raid took place.

That weekend, his team back in the States, back at the old Naval Ordnance Lab, had been doing a game on a Japanese minelaying attack of Pearl Harbor, the Japanese coming in and laying sea mines in the harbor to tie it up, and that occurred the same weekend that the bombing raid occurred.

MR. THOMAS: At an ORSA meeting in Pasadena, I gave a paper on some of our conclusions about the role of gaming, and Ellis Johnson was in the audience. I guess he thought we were a little harsh. I think he was a bit hurt by what we said, and we felt that gaming has a very useful role, but Nick Smith at ORO had written that gaming was a method of solving games, and we felt that it was very limited in that respect. Nick Smith had been an Air Force analyst in World War II in very useful studies of interdiction.

On your checklist you asked about the impact of common sense. It was very useful in checking the results of models, especially when one had any experience on that weapon system design and development. I think there had been a loss of that to some extent with the onset of computer programming, complicated, detailed simulation models.

Many people became big on simulation results as being more definitive than, in fact, they are, and some de-emphasis of appreciation of the importance of the analyst in exercising common sense and intuition resulted.

DR. SHELDON: Were many of the analysts that you worked with back then what we call subject matter experts, now SMEs, with combat operational background on the OR teams?

MR. THOMAS: There were some who had experience that was applicable, and that was very useful, and the few who had had OR experience in World War II who stayed on were very useful in educating their successors.

It was really in the fifties, I think, that people began to look on OR as a profession. ORSA was founded about '52, the Military Operations Research Society, MORS, in '57, and the first few ORSA presidents all had had World War II experience, and that showed, of course, in these panel discussions, on definitions and discussions of training.

The interest in professional societies was partly motivated by academicians who had worked in World War II OR, and came back to their academic specialties, but they had not published a great deal in their pure subject matter, but had published important papers in applications of operations research. The new societies gave them an opportunity to be recognized, as well as the value in passing on that knowledge to others, in an exchange of information in societies and their publications, and there is still a valuable opportunity for professional societies today, but the public places more of an emphasis on some of the techniques at the expense of the applications.

It was interesting to see how military OR groups were organized. It was partly by subject matter, partly by the techniques and methodology that would be exercised by a subgroup of the group and partly to meet administrative or liaison type needs.

In the Air Force Operations Analysis Office, where I went in 1955, to work for Walt Deemer, Roy Brothers was the Assistant for Operations Analysis with two-star equivalent rank. We had five divisions that were called teams in those days. Two of them were organized, in a sense, to fit new techniques.

One was based on atomic energy, and one was based on guided missiles, new weapon technologies. One of the groups, the one closest to classical OR, was the Combat Operations Team headed by Hugh Miser, that was involved in analyzing exercises.

We had a planning analysis team that I was on, headed by Walt Deemer to support the Air Force headquarters staff people working on future combat forces by making studies of future weapon systems and their impact on our relative military potential.

Our fifth team was a liaison team. There was an Air Force Regulation 20-7, which governed the development of operations analysis. If one of our major commands wanted a new operations analysis office, they went through the procedures outlined in AFR 20-7, and similarly if they wanted to disband the group. That rarely happened. The Liaison team helped with the implementation of AFR 20-7.

MR. VISCO: Was there much interaction between the planning analysis group and the combat operations group?

MR. THOMAS: Yes, there was considerable interaction. Exercise results served as a guide to what future systems might be capable of, and as a matter of fact, there was quite a bit of interaction for the same reason among the dozen or so offices that existed under 20-7.

We had meetings about twice a year, technical seminars, usually held at an Air Force base, and there was a lot of exchange of information, and that was very valuable.

MR. VISCO: At those exchanges, did you work with some of your Army and Navy counterparts there or was that still primarily Air Force?

MR. THOMAS: That was primarily Air Force, but after MORS was founded in 1957, it provided a forum for joint participation.

In 1971, the Air Force OA Office was merged into a large office, the Air Force Studies and Analyses Office, and AFR 20-7 gradually fell into disuse. There were bilateral arrangements, but fewer Air Force exchanges of the kind that the technical seminars had provided. It was partly, I suppose, because MORS was developing in those days and provided an avenue for joint participation of Army, Navy, Air Force.

The impact of some of the early arrangements was probably more to serve as examples and to provide the initial condition for the development of later groups, but there were quite a few changes.

I think some of the excitement was probably lost in the days when there was less pioneering required than in the earlier days, although anyone who was newly becoming acquainted with operations research would still find that excitement up here.

Of course, in those days, there were various sources of funding. Project CHORE had been funded by the three services initially, and later it became solely Air Force sponsored and funded by the early fifties. The Air Force operations analysis offices were funded by the normal air staff procedures for budgeting and funding.

Other assets required things like data sources, and they required suitable clearance for access to databases. There was considerable interchange among analysis groups, one group learning from another, taking advantage of sites where experience had been gained already.

There is considerable support of analysis groups as long as there is still a tie to the initial sponsor. Sometimes, as time went by, those ties may be eroded and sponsorship might be less enthusiastic in coming.

As long as the groups were considered valuable by someone who can control funds, there was pretty adequate funding. So, one can see that some consideration of the needs of high level management is very important. If an operations research group evolved to meet the needs at the time, it was likely to be funded at least for some time. If its usefulness gradually disappeared, then the group might go out of existence, and, of course, operations research groups in industry had some of the same effects.

DR. SHELDON: How did you see the operations research groups evolve from, say, the Korean War to the Cold War in between there and the Vietnam War? Did you see an evolutionary process or a change in emphasis, funding sources, and methodologies?

MR. THOMAS: I think the biggest change during those years was in methodology. As I said earlier, many people became infatuated with simulation, attributed to it over-estimated reliability, and there was some neglect of fundamentals.

Some of the early methods made use of observation measurement and experiment to good advantage, and there is a very good paper

by Omand Solandt entitled *Operation, Experiment, and Measurement in Operations Research.*

The early calculating powers of the first electronic computers were quite limited. When I was in CHORE in Chicago we sent some analysts to the ENIAC to calculate some pseudo-random numbers that were useful in some of our work but the power of the ENIAC at that time was no greater than what we can now carry on our wrists.

I think the influence of the early techniques on later techniques was probably strongest in the area of systems analysis for the same reason the systems analysis developed initially.

When you didn't have that kind of data that you get from recording the results of actual operations, you have to resort to other devices, experiments, tests, conjecture, extrapolation, operational results, so that the systems analysis is still valuable today.

We still have many of the same limitations, which are perhaps strongest in the area of human behavior and human performance although now, of course, we have distributed interactive simulation, which allows one to get some human inputs there. It is not clear how similar human behavior in DIS is to behavior in actual combat, but probably closer than conjecture is.

Some of the techniques that have come into existence may be valuable in getting some useful experiments. A lot of the early institutionalization of OR groups involved those that had been doing experimental work either in World War II or in prior quantitative work. Lots of post-war analysis groups had evolved from World War II groups, some of the early institutionalized groups. We had examples, such as the Army group at Aberdeen Proving Ground, Maryland, the Navy's Operations Evaluation Group, OEG, that evolved from some of the ASW work in World War II, and the Air Force operations analysis group in Air Force headquarters that had analysts who had been in World War II analysis groups in the field. Some who had been involved in World War II analysis came to Wright Patterson Air Force Base and were working after the war in some of the laboratories there.

MR. VISCO: Were the early folks at RAND from the same kind of backgrounds? Since RAND was formed in '47, or thereabouts—I can't recall who the early people—

MR. THOMAS: I think that more of the people at RAND came from academic sources after evolution from the original Douglas Aircraft group. There was some sprinkling of, quote, "practical," unquote experience.

MR. VISCO: That was my impression also.

MR. THOMAS: But RAND, I think was good at establishing contact with the people who were operating modern equipment and picking their brains, and, of course, there were groups in a lot of the aircraft companies, some of the companies that worked on electronics, and so they all had pretty good OR groups.

One of the early contracts, one of the very few early contracts that the Air Force Operations Analysis Office had, was with Northrup, and by that time, Herb Weiss had left Aberdeen and was no longer working for the government, and he had a group at Northrup. In such groups there was difference in approach from one analyst to another, and some difference from one agency to another.

I recall that in Project CHORE, I had a chain of inequalities involving five analysts where Analyst A was more theoretical and less practical than Analyst B, Analyst B had the same relationship to Analyst C, etc. Each of the five, however, was doing valuable work and was really used appropriately, to take advantage of his obvious strengths of one kind or another.

I recall that Les Dubins, who was co-author of a paper with Jimmy Savage, entitled *How to Gamble if you Must,* a valuable analysis of gambling, would often come into the room and start a discussion by going to the blackboard and saying suppose we have a space X, a Borel field B, and a measure mu.

MR. VISCO: What was his name again?

MR. THOMAS: Les Dubins. I recall one day I brought my daughter to work, and Charley Price, whose Ph.D., I think, was in algebra, came into my office, and started talking about some math problem, filling the blackboard with equations.

My daughter was very discreet, and waited until he left, to say, "Dad, is he allowed to fritter away his time in mathematics?" I had always thought that mathematics was quite useful.

My early trips, as I think I indicated, were motivated by the desire to pick the brains of some other organization, including the January '48 trip to Aberdeen, and the December '48 trip to RAND, where I touched base with some of their game theorists. Of course, general rela-

tionships of organizations, one learning from another, led to many trips.

DR. SHELDON: Did you take any of your OR skills to the combat theaters in Korea or Vietnam to do some analysis?

MR. THOMAS: Yes, our Chicago group sent some analysts to Korea, and the Air Force operations analysis office sent some to both Korea and Vietnam.

DR. SHELDON: What kinds of things did they study in the Korean theater?

MR. THOMAS: They studied much the same thing that had been done in World War II (radar effectiveness, interdiction operations, fuse settings, etc.). As a matter of fact, there was a rediscovery of things that had been lost, things that had been known from analysis from World War II, and then rediscovered in Korea, and then later sometimes in Vietnam.

MR. VISCO: The Air Force, during the Korean War, did they organize elements the way they had organized them in World War II, that is, analysts directly in support of a unit of the Air Forces? I recall there were groups with various bomber commands during the war, but I don't recall anything like that in Korea. Do you?

MR. THOMAS: There was some analysis at that time, mostly done by some of those that had some World War II experience or knew of it more intimately, but some of the gun camera film analysis was actually done in Project CHORE back in Chicago.

MR. VISCO: In comparison with the ground operations: before the Korean War was over, over 50 percent of the analysts at ORO had actually served in Korea. ORO at that time had about a hundred people, but still that is a lot of analysts who spent time in Korea during the war.

MR. THOMAS: Well, Roy Brothers, head of Air Force OA, was quite impressed by the difficulty of getting analysts to go to Korea. Many of them didn't see it as terribly important relative to World War II.

As a matter of fact, there was more interest that surfaced in 1957, after the Korean armistice had been signed, long past that, when the Sputnik went up. I remember getting a phone call in 1957 from Dave Votaw who had been a World War II analyst, and he said, "Do you think I should come back to work?" Very little of that happened in Korea. So, what Roy Brothers had done because of his Korean difficulties was to establish four standby units at different universities: one in North Carolina, one at Iowa State, one at University of Pennsylvania, and one at the University of Denver.

His concept had been that each of those constituted a team, composed of analysts of different disciplines, that would become familiar with what was going on nowadays or "thenadays" in operations research, and in event of war could be sent to the theater. However, the teams never were used as he had visualized. They were useful in reviewing papers, refereeing journals, sometimes doing a bit of theoretical work, but they were never actually brought to bear in Vietnam or any other combat area.

I have indicated what some of the key institutions were in the early days and how they interacted. I don't know that there was a great deal of impact on later work. I don't know that the differences between agencies had too much impact on the work. I think there was probably more impact that depended on the leaders to whom an OR agency reported. If the agency reported to somebody who appreciated the role of operations research, and if the leaders found the results of the agency useful, it could be a very fruitful relationship. However, if there was rotation and a new general officer came in, and he was not aware of the value of analysis, and if the operations research group was not good at communicating the value to the leaders, then, there might be relatively little use made of that group or it might be actually serving other staff agencies to some extent.

MR. VISCO: Do you have any examples from your early days in MORS, in its early days of learning some institutional differences between Army, Air Force, and Navy analysis?

MR. THOMAS: Well, I don't know that I learned so much from MORS. There were some differences that I noticed while still at Chicago before MORS had been founded, and I saw that many of the differences depended on the leadership of the groups.

The OEG, I think, continued to be more devoted to theory than the Air Force lead office was in the early days, the lead office being more devoted to experience, experiment, exercises, and so on. However OEG did have one valuable practice; it sent different analysts out to the fleet, so they had hands-on experience with actual operations. That was something that other groups could emulate.

MR. VISCO: Clay, do you have any feeling about was it better in the early days than it is now? Clearly, it is different. Organizations

have changed considerably from the forties and fifties, but can you reach any kind of value judgment as to quality of work, ease of working with people, making more of an impact, just some general observations about then and now kind of thing?

MR. THOMAS: I think that to a large extent there is more specialization now. Many people in operations research offices have become very expert on some simulation program, a good deal of their time has gone to involvement in studies that use that computer program. Some of them developed good contact with those people in the OR shop who have had actual operational experience, and that's a valuable interchange, but without that, sometimes there is overspecialization, and journal editors become overly academic, and show little appreciation for a scientific approach.

I will give an example. In MORS, you mentioned, there has been some considerable interest in the so-called new sciences, chaos, complexity, and so on, and my feeling is, as I have expressed at MORS meetings, there is too little appreciation for the classic scientific method itself, as opposed to some theoretical qualities.

MR. VISCO: Was it more fun in the early days than it is now? Do you enjoy it more?

MR. THOMAS: I think that when one is working on a good problem and making progress, that is always fun. I think in the early days it may have been easier to find things that had not been done at all.

Today, there is some possibility that one will rediscover something that was well known 20 years ago, but had fallen into disuse or neglect. There are still plenty of unsolved problems, like discovering the proper role for techniques like distributed interactive simulation DIS, or ADS, advanced distributed simulation. That is still a challenge, and learning more about how people perform in combat as compared with performance not in combat is still supremely challenging.

MR. VISCO: There is one last question, and that is, what would be the most important advice you could give to someone just starting out in military operations research today?

MR. THOMAS: My advice would be ask yourself if you really have a passion, if you really get excited by applying a scientific method, quantitative methods, in areas where they had previously not been much used, and if you do have that passion, that excitement, then, by all means consider a career in operations research. If you are not excited by it, I would say you might be more interested in some special discipline, maybe computer programming or some academic discipline.